Stable Isotope Approach for Long−Term Monitoring of Changes in Precipitation Source Temperature and Sierra Nevada Snowpack Runoff

M. Lee Davisson

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0311: Stable Isotope Approach for Long−Term Monitoring of Changes in Precipitation Source Temperature and Sierra Nevada Snowpack Runoff

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Dr. Davisson and his colleauges have submitted a very interesting proposal for monitoring the isotopic variations in the Sierra Nevada snowpack. This proposal will provide useful information on both the onset of snowmelt and the moisture sources of precipitation. The applicants also state that the isotope monotoring will be useful for streamflow forecasting, but they do not provide convincing arguments on how this will be done. The isotope monoitoring may be useful for model calibration, to identify areas for hydrologic model improvements, and provide data for testing new snow cover parameterization schemes, but the primary panel reviewer struggled to understand how the monitoring activities will improve streamflow forecasts directly. Nevertheless, the proposal is worthy of funding because of the new data collected. Pertinent external reviewer comments: (1) This is a totally new approach to analyzing snow accumulation and melting. (2) Since the researchers have done intial sampling and testing, they have presumably ironed out most of the bugs in the procedures and are well prepared to conduct a much larger scale test, and then go on to fit the data to modeling. (3) One reviewer had serious and lengthy concerns that the applicants were oversimplifying complex processes, and that their sampling strategy could not elucidate pertinent details

of the isotopic signatures.

Additional Comments:

Dr. Davisson and his colleauges have submitted a very interesting proposal for monitoring the isotopic variations in the Sierra Nevada snowpack. This proposal will provide useful information on both the onset of snowmelt and the moisture sources of precipitation. The applicants also state that the isotope monotoring will be useful for streamflow forecasting, but they do not provide convincing arguments on how this will be done. The isotope monoitoring may be useful for model calibration, to identify areas for hydrologic model improvements, and provide data for testing new snow cover parameterization schemes, but the primary panel reviewer struggled to understand how the monitoring activities will improve streamflow forecasts directly. Nevertheless, the proposal is worthy of funding because of the new data collected. Pertinent external reviewer comments: (1) This is a totally new approach to analyzing snow accumulation and melting. (2) Since the researchers have done intial sampling and testing, they have presumably ironed out most of the bugs in the procedures and are well prepared to conduct a much larger scale test, and then go on to fit the data to modeling. (3) One reviewer had serious and lengthy concerns that the applicants were oversimplifying complex processes, and that their sampling strategy could not elucidate pertinent details of the isotopic signatures.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

While the primary reviewer rated this proposal as above average, one reviewer raised a number of substantive comments, some of which, if addressed, could improve the proposed research. The panel also had significant concerns. These included the linkage of the isotope data to stream hydrology, and the utility of the proposed research for improving streamflow forecasting.

Technical Review #1

proposal title: Stable Isotope Approach for Long−Term Monitoring of Changes in Precipitation Source Temperature and Sierra Nevada Snowpack Runoff

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals and objectives are clearly set forth in the project purposes section. The idea of using isotopesto track the melting phase of snow would be a new approach and might lead to better runoff forecasting. Given the possibility of significant global warming with its impact on Sierra snowpack, this could be another internal tool to measure change, especially in the timing of initial snowmelt. The project may also shed some light on the moisture origin for the winter snowpack.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The authors have done enough work previously to settle on a methodology and theory based on some initial sampling which looks promising. An expansion of sampling is the next logical step and it appears that they will have the support of a number of field workers in cooperating agencies to achieve their goal at minimum**

	sampling costs.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**This is a totally new approach to analyzing snow accumulation and melting. Establishment of a current typical pattern on a substantial scale, as planned, would enable comparisons in future decades of changes expected from global warming. Global warming, depending on the amount, could have a significant on the California mountain snowpack, currently an important element of natural storage for our water supply.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Since the researchers have done initial sampling and testing, they have presumably ironed out most of the bugs in procedures and are well prepared to conduct a much larger scale test, and then to go on to fit the data into modeling. Weather conditions from year to year can be quite variable; it may take more than 2 years to confirm the hypothesis and its range of coverage, depending on the kind of years we get during the project period.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The desirability of continued monitoring would depend much on results of the two year measurement program. The methods may lead to operational continuation of such sampling, probably on a smaller scale at certain times of the year most useful to water supply forecasting. Or the project could set a reference level to be checked in 10 years or so to see if there has been significant changes in the accumulation and depletion phases of the snowpack with anticipated climate change.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The authors offer the possibility of this methodology being used to improve operational forecasting of snowmelt runoff and timing. This reviewer is not convinced the method will be that useful in real time and with limited budgets. But the potential for identifying changes in snow composition and melt timing by comparing the current situation with future years can definitely help assess impacts of expected climate change.**
Rating	**very good**

Additional Comments

Comments	**As a totally different than conventional method of measuring snow-- the accumulation, possible sources of the moisture, and timing of the onset of melting--, this approach deserves support. Its potential is large.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**I have no direct knowledge of the authors but they appear to be well experienced in their fields. Of note is the fact that they have obtained the respect and help of cooperators who will be helping a lot in the field sampling work.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The cost is much more reasonable than others I have looked at. Since the researchers have done initial work to perfect their methodology, their estimates should be pretty good. They will save a lot by getting the assistance of other snow cooperators in their field sampling.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**I think the proposed project will provide some valuable new information about the origin of moisture**

	and the accumulation and melting of our Sierra mountain snowpack and a base condition upon which to measure changes during the coming decades. Costs are reasonable and the model development may eventually lead to better operational runoff forecasting.
Rating	**excellent**

Technical Review #2

proposal title: Stable Isotope Approach for Long−Term Monitoring of Changes in Precipitation Source Temperature and Sierra Nevada Snowpack Runoff

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**In turn,** **Goals: The stated goals are very ambitious and it is very unlikely that they would all be realized over the term of this proposal. While I agree that the project could certainly contribute to the theory and the observational data base of snowmelt generation; its ability to contribute new methods that facilitate hydrologic forecasting in the Sierra Nevada is over−reaching in my view based on the current knowledge in the field, the outlined interpretive obstacles and the scale of this proposal.** **Objectives: Objectives a,b and c listed in section 1.4 are reasonable based on the stated approach, objective d is not.** **Hypotheses: The hypotheses are listed only once in the proposal, within Fig. 1. While two hypotheses are listed there, only one is germaine to this proposal. It is clearly stated and internally consistent.** **Is the idea timely and important: Yes, absolutely. The ability to accurately predict, even over the short term, the amount of freshwater derived from snowmelt would materially benefit hydrologic planners. Additionally, the prospect of being able to use this technique, or a similar one, to investigate the longer**

	term effects of climate change on annual snow accretion and melt would be both an important scientific endeavor and powerful long term management tool. As discussed in more detail below, it is the planning of how to execute this proposal that falls short, not the idea.
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**In turn,** **Is the study justified relative to existing knowledge: The study as proposed is very ambitious in scale and application. While the authors emphasize the relatively low cost of these analyses, they under emphasize the logistical complexity inherent in their approach in terms of spatial and temporal sampling scales in particular. The more obvious complications associated with this work may take years of data collection over a range of areas to eventually, if ever, be adequately addressed. The authors do acknowledge knowledge gaps, but it seems evident that these gaps must be filled in order for the hypothesis of the proposal to even be addressed.** **Is the conceptual model clearly stated and does it explain the underlying basis for the proposed work: The conceptual model is clearly stated and it does explain the underlying basis for the proposed work. The authors wish to collect enough data to calibrate and validate a predictive model, which, if successful, would then be used to predict onset of snow melt and total expected volumes of freshwater supplied by snow melt to reservoirs within the Sierra Nevada.**

	Is the selection of research, pilot or demonstration project, or full−scale implementation project justified: At this stage, a research project selection is most appropriate. More data is required to further refine the proposed technique.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**In turn,** **Is the approach well designed and appropriate for meeting the objectives of the project: No. The proposal is divided into two basic parts, the description of what they want to do and how, and the description of the model they intend to use and how the data collected in the first part will serve the needs of the second. The idea, need for the work and modeling portions of the proposal are well written and presented, however the portions of the proposal that address the complexities in the methodology (i.e. what processes effect isotope signatures and how to deal with them) are significanlty lacking as is the sampling protocol, which is only briefly addressed. I found this surprising, since the type, frequency and location of sampling is of primary concern if the authors intend to successfully address these complexities when it comes time to interpret their data.** **Is the approach feasible: As written and presented, no. The approach leads one to some obvious questions... 1. The authors cite a significant amount of work that details the many processes that alter isotopic signatures of snowpacks (recrystalization,**

phase change, evaporative losses and rain on snow), but then propose that since large scale homogenization seems the case based on their data and groundwater data of Rose et al. (1996), these variability producing processes can be ignored? They propose a simple numerical average approach for delta 18−O data for samples collected over the entire snow pack, essentially anywhere in the Sierra Nevada, at the watershed scale, including those with long reservoir storage and long turnover rates. It seems this approach is an over−simplification. 2. The authors cite preliminary data (theirs and Rose et al. 1996), that clearly show that progressively increasing elevation results in increasing depletion of delta 18−O. They then discuss how early runoff is isotopically depleted relative to the in place snow due to fractionation, and that as the snow melt progresses, both the snow and resulting runoff undergo isotopic enrichment as required by mass balance. They then make the statement that runoff data show that these mass balance effects outweigh elevation driven effects? How can they determine which has a greater effect and at what scales (temporal and spatial)?

Are results likely to add to the base of knowledge: There should certainly be some results that add to the base of knowledge, yes. The sampling methodology and ability of the authors to adequately address the stated complexities are the chief weaknesses of the work as proposed.

Is the project likely to generate novel information, methodology or approaches: As presented, no. This is an important idea and needs additional refinement. A better designed approach would have much higher prospects for success in terms of novel information, methodology and future applications.

Will the information ultimately be useful to decision makers: Most likely not. There is more than one reason for this that should be noted. One is the shortcomings

	in design of this particular approach. Another is time, as these authors point out. It will require years of sampling over a large range of areas to generate the type of database that would allow for confident predictions to be made regarding these complex processes. Only after sufficient database maturation, would this approach be of routine use to decision makers. This is not to say that some investments towards this end are not justified.
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**In turn,** **Is the approach fully documented and technically feasible: Yes and no. The approach in terms of the science behind the determination of these istopes in water is well documented and technically sound. I have no doubt the authors are capable of generating quality data. The approach in terms of how the study is designed to meet its objectives is not fully documented and does not appear to be technically feasible. For example, the authors list an extensive array of collaborators who will be charged with the bulk of the sampling responsibility. Yet, they do not outline in detail how this sampling is to be carried out? There is no map showing sample locations, they mention high frequency sampling events (days, weeks, hours, etc.), but have no supporting design as to how specific sampling regimes would serve to address the methodological complexities they outline as inherent in this work?** **What is the likelihood of succes: As presented, the likelihood of success is low.**

	Is the scale of the project consistent with the objectives and within the grasp of the authors: No. The objectives are far reaching and overly ambitious with respect to the design of the approach. While the authors are certainly competent scientists, they differ the sampling and apparently the design of that sampling, to others who may not be as familiar with the methodological complexities inherent in this work. This is a major shortcoming.
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring is not applicable. There is no discussion in the proposal of pre−post or treatment−control comparisons. There are no stated plans to interpret monitoring data or otherwise develop that information. The second of the listed hypotheses in Fig. 1 implies long term monitoring, but this effort is not articulated in the text of the proposal.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**In turn,** **Are products of value likely from the project: There would certainly be some data gathered which would be of use. The likelihood that products would result that**

	could be of immediate use to decision makers is very low. **Are contributions to larger data management systems relevant and considered: Yes, they are relevant but no, they are not articulated as planned in this work. The authors state that long term database growth and management is necessary to realize the long term goals of this work, but do not explain how this would be realized.** **Are interpretive (or interpretable) outcomes likely from the project: Most likely, not. The design of the approach is not sufficient in my view to overcome the methodological complexities.**
Rating	**fair**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**In turn,** **What is the track record of the authors: The PI and his co−investigators have a very good track record in terms of their past performance in research.** **Is the project team qualified to efficiently and effectively impliment the proposed project: In terms of laboratory work and modeling, absolutely yes. The team is lacking a well qualified field scientist, and this is reflected in their reliance on collaborators to conduct sampling and in the poor design of the sampling methods.**

	Do they have available the infrastructure and other aspects of support necessary to accomplish the project: Yes.
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Yes, the budget is both reasonable and adequate for the work proposed. I suspect however that a better designed and more holistic sampling program would have resulted in an increased budget. This would have been funds well spent and would have resulted in a stronger proposal overall.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**My summary rating is based on the mean of all ratings for each review category.**
Rating	**good**